The gold obtained from mercury seems to be mostly adsorbed to carbon. Ruby glass is formed by heating small pieces of glass with the carbon; in the process now used it is formed in numerous spots on the walls of the distilling flask by repeatedly heating it to about 600°. We have often separated mercury by washing the oil with benzene and ether, and after separating it from carbon by centrifugal separator, distilled it in vacuum and examined the

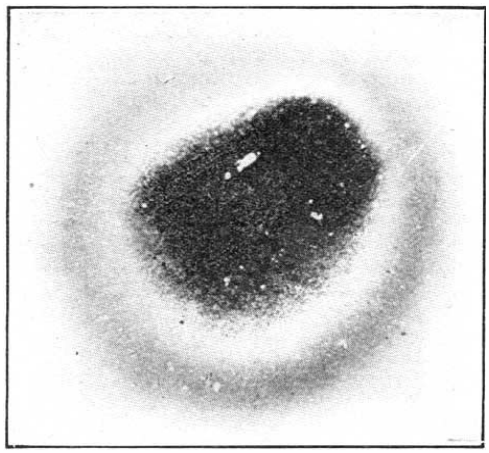

FIG. 2.—Ruby glass by transmitted light. ×150.

residue, which generally contained no gold, but a minute quantity of white metal, which may probably be another product of heavy discharge; it was, however, too small to be tested chemically.

The accompanying illustration (Fig. 2) shows a spot of ruby glass photographed with transmitted light and magnified 150 times. The central dark portion contains gold particles distributed as shown in Fig. 3 taken with reflected light and magnified

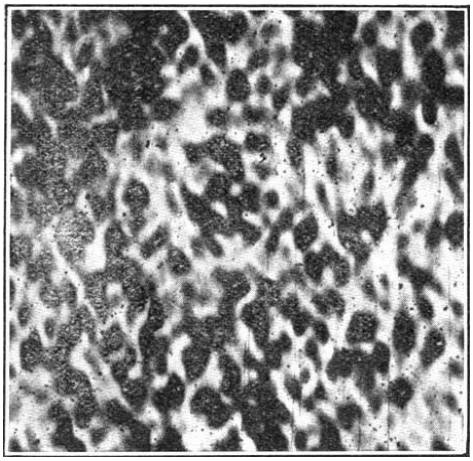

FIG. 3.—Ruby glass by reflected light. ×2500.

2500 times. It represents only a small part near the boundary of the central spot. The white ring is greenish blue, and the lightly shaded one is rosy; these colours are characteristic of gold colloids. Numerous spots of this kind are obtained in the bottom of the distilling flask during the after-treatment of a mixed mass of carbon and mercury after heavy condensed discharges. Sometimes ruby glass is apparently covered with a thin film of gold; on microscopic examination it is found to consist of fine particles of gold very densely distributed. The primary current in the induction coil in such experiments ranges from 25 to 30 amp., and the spark length in air is more than 1 m.

Probably we could produce the same effect by using lower voltage, if sufficient capacity were inserted, but the resistance of the vessel is not great enough to withstand the heavy discharge, especially when carbon and mercury are intimately mixed together. The construction of a proper discharge vessel seems at present to be a difficulty in getting an amount of gold sufficient to determine its atomic weight. Which of the isotopes of mercury is changed into gold can perhaps be inferred from the atomic weight. Spectroscopic examination will be started so soon as we can obtain sufficient material for the purpose.

The process taking place may be looked upon as due to commotion in the nucleus by intense electric force. If we assume that Coulomb's law ceases to hold within the nuclear boundary, the positively charged protons form a compact core, and the electrons within the boundary surround it. On applying an external electric field the motion of the core is opposite to that of the electrons, so that if the field be sufficiently strong, it is possible that some of the electrons may pass out of the nuclear boundary, and if the core be not very stable, some of the protons constituting it may get out. The commotion thus introduced by the external force will have some resemblance to radio-active disintegration, which must be attributed to the internal commotion of the nucleus. An experiment was made with ferro-uranium, to see if the radio-active process cannot be accelerated by applying a strong field, but owing to the ionisation it was difficult to maintain the field for a sufficient length of time. An investigation of the process of accelerating the disintegration must, therefore, be reserved for future experiments.

The experimental procedure here sketched cannot be looked upon as the only one for effecting the transmutation; probably different processes will be developed and finally lead to industrial enterprises. At present, there is no prospect of producing gold economically from mercury. Experiments with various elements may lead to different transmutations, which will be of significance to science and industry. Meagre as is the result, I wish to invite the attention of those interested in the subject so that they may repeat the experiment with more powerful means than are available in the Far East.

H. NAGAOKA.

The Institute of Physical and Chemical Research,
Komagome, Tokyo, May 26.

The Quantum Explanation of the Zeeman Triplet.

IN his letter published in NATURE of June 27, p. 978, Prof. W. M. Hicks raises some interesting points in connexion with the quantum theory of the simple Zeeman effect. As Prof. Hicks points out, the theorem of Larmor's usually taken as the basis of the theory does not define in any manner the relation between the orbits on which the rotation is superposed in the presence of the field on one hand, and the corresponding orbits before the imposition of the field on the other. The supposition that these two sets of orbits are identical is, therefore, in no way justified on the basis of Larmor's theorem alone. It can, however, be shown from purely classical considerations (see G. A. Schott, "Electromagnetic Radiation," Cambridge University Press, 1912, §302, p. 317) that, to the first order in terms involving the field, the two sets of orbits are identical. Schott's proof takes into consideration the induction

forces which act on the moving charges during the period of establishment of the field, whereas Larmor's theorem confines itself to the so-called Coriolis forces which, as Prof. Hicks points out, act transversally on the moving charges, and hence cannot alter their energies.

Prof. Hicks proves in a simple case that the application of the Wilson-Sommerfeld quantum conditions to the Bohr hydrogen atom with reference to fixed axes (instead of the special rotating axes employed in the usually accepted theory) leads to no Zeeman effect at all as a first approximation. A more general proof of this was given in a paper of mine about two and a half years ago (Roy. Soc. Proc., A, vol. 102, 1923, p. 529) in which I also put forward an alternative theory of the simple Zeeman effect which seems to me to answer Prof. Hicks's purpose. The theory is based on a slightly extended form of the quantum conditions which was first suggested by Prof. William Wilson (Roy. Soc. Proc., A, 102, 1923, p. 478), namely,

$$\int_0 \pi_i dq_i = n_i h, \qquad (i = 1, 2, \ldots)$$

where $\pi_i = p_i + eA_i$,

p and q being the usual Hamiltonian co-ordinates, e the charge on the particle in question, and A the generalised magnetic vector potential. These conditions are applied both in the absence and in the presence of the field, thus defining the orbits and their energies in both cases, and the frequencies are then obtained from the energy relation $\Delta W = h\nu$. It is also shown that the relation between corresponding orbits defined by the extended conditions (*i.e.* orbits for which the quantum numbers are the same) is in complete accord with Schott's theorem; in fact the latter is derived as a necessary consequence of the quantum conditions themselves.

A. M. MOSHARRAFA.

The Manor House,
Alphington, near Exeter, June 28.

THE objection of Prof. Hicks to the use of Larmor's principle (NATURE, June 27, p. 978) is well founded, but the Zeeman triplet effect can be made to fit into the quantum theory by keeping strictly to dynamical principles. The phase-integral $\int p dq$, for a variety of reasons, is, for the case of a magnetic field, to be replaced by $\int (\delta L/\delta \dot{q}) dq$, where L is the Lagrangian function. For the hydrogen atom

$$L = \tfrac{1}{2}m(\dot{r}^2 + r^2\omega^2) - \tfrac{1}{2}Her^2\omega/c + e^2/r.$$

Hence, on quantising,

$$mr^2\omega - \tfrac{1}{2}Her^2 = nh/2\pi.$$

From this, for radial quantisation,

$$m^2\dot{r}^2 + n^2h^2/4\pi^2r^2 = 2e^2m/r - 2mC',$$

where $C' = C + nhe/4\pi mc$, $-C$ being the energy and H^2 being neglected. Hence the "permitted" value of the energy is

$$-(2\pi^2me^4/h^2)/(n+n')^2 + nhe/4\pi mc,$$

where n, n' are the azimuthal and radial quantum numbers.

ARTHUR W. CONWAY.

Abbeyview, Dalkey, Co. Dublin,
June 27.

The Oogenesis of Lumbricus

IN a letter to NATURE (June 27, p. 979) Prof. J. B. Gatenby objects to certain comments upon his work made recently by Mr. L. A. Harvey in a paper on yolk-formation in the earthworm (*Q.J.M.S.* 69, p. 291). Mr Harvey is a student working in this department and it is on his behalf that I wish to protest against the tenor of Prof. Gatenby's letter. It is quite evident that Prof. Gatenby has not comprehended clearly the contents of Mr. Harvey's paper; for his letter contains misstatements, and these may do a considerable amount of harm unless speedily contradicted.

Prof. Gatenby accuses Mr. Harvey of having been discourteous in saying that a glance at a paper of his (Prof. Gatenby's) summarising what is known about the formation of yolk shows that "really very little is known" on the subject. Mr. Harvey was perfectly justified in making this statement—it is simply a statement of his opinion—and on this point I am in complete agreement with him. The fact that Prof. Gatenby disagrees with the statement does not make it discourteous. The paper referred to, Prof. Gatenby complains, is an "old one." Its actual date is 1920, and if the advance since then is represented in Dr. Brambell's paper (1924) on "Yolk," to which Prof. Gatenby refers, it can safely be said that any advance made has been extremely small.

The remarkable objection is then made that Mr. Harvey, in studying yolk-formation in Lumbricus, is not justified in inferring any conclusions as to the similar process in Limnæa—a form studied by Prof. Gatenby. He gives no reason in making this statement. However, he previously refers to a paper by a student of his as containing an account of *Molluscan* oogenesis. Actually it deals with two forms and those both gastropods, and hence any general conclusions drawn must have been inferred from the study of those two forms.

Prof. Gatenby suggests that before criticising his work Mr. Harvey should have repeated it. While I admit that repetition might be desirable, it is obvious that Prof. Gatenby has failed to grasp Mr. Harvey's criticism, which is, not that his observations are at fault, but that his deductions are. This is made perfectly clear on p. 292.

Prof. Gatenby's next point is that it was unfortunate that the egg of Lumbricus was chosen for the study of yolk-formation, as it contains no "real yolk." This is incorrect. Yolk *is* present in the egg, and the criteria used for the recognition of that yolk were those advocated by Prof. Gatenby himself. This is fully explained on p. 299. Further, Prof. Gatenby objects that Lumbricus is a "special atypic annelid" and yet refers to Saccocirrus (apparently) as a typical annelid.

It is the static conception of the cell to which Mr. Harvey objects. He regards it essentially as a dynamic concern—an equilibrium system in which the constitution of each constituent is a function of its surroundings—and because of this he considers that the technical methods and the reasoning adopted in modern cytological investigations into the question of yolk-formation are wrong. If Prof. Gatenby had read more carefully the introduction to Mr. Harvey's paper he would have grasped this, and, in that event, it is to be hoped, would not have written his letter.

H. GRAHAM CANNON.

Zoology Department,
Imperial College of Science,
South Kensington, July 2.

Transmission of a Rosette Disease of the Ground Nut.

THE important part played by insects in the dissemination of the virus diseases of plants is now recognised, and experimental proof of transmission by particular insects exists in a number of cases. As a result of investigations during the past season, we are able to add one more to the list of those diseases of which the insect vectors are known.